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Reply

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Sun's comments question three separate things: (a) the principal conclusion of the subject paper that the Australian tektites came from the moon, (b) the suggestion of another paper [Chapman, 1963] that they might have come from the large lunar crater Tycho, and (c) the reliability of the particular atmosphere-entry trajectories deduced from aerodynamic analysis in these two papers. The reply which follows is formulated in the respective order of these three separable topics.

a. Sun's formulation of an 'impasse' to the theory of lunar origin of tektites, as outlined in the third numbered paragraph of his comments, appears to be based on a fundamental oversight about the physics of fluid drop distortion. He states that, since the moon's gravitational acceleration is 0.165 earth g , the deceleration of the molten glasses also would be 0.165 earth g , which is over ten times greater than the deceleration of 0.015 earth g for which aerodynamic forces would prevent the formation of molten spheres of australite size; and this, therefore, constitutes an impasse to the theory of lunar origin of tektites. Sun has simply overlooked the fact that, once the material is ejected from the moon, the gravitational forces act *uniformly* on each volume element within the blob, and therefore do not provide any forces producing either distortion of that blob or disruption. (The slight differential in gravitational attraction from one side of a blob to the other is indeed trivial.) Aerodynamic forces, in contradistinction, act nonuniformly over the drop surface, and thus can produce distortion and disruption. The fact that a small molten drop moves in space within the gravitational field of a celestial object of 0.165 earth g , or 16.5 earth g for that matter, is irrelevant to the shape which the liquid drop finally takes as it solidifies during its travel through space in the weightless state. Thus I fail to see any foundation whatever to this particular impasse to the theory of lunar origin of tektites.

b. In regard to Sun's view that it is very difficult to see how molten material could have escaped severe distortion while passing through the gases created during the formation of a lunar crater as large as Tycho, I would like to note that we do not maintain that all tektites have been undistorted by their passage through such gases; on page 4320 of our paper we suggest, in fact, that the tektites in southeast Asia possibly have been shaped in such a manner. The flattened and stretched forms commonly found in this area, and also in other tektite strewnfields, are much more numerous than the australite forms which are not flattened or stretched; hence, shaping during passage through the blast of impact gases might affect the majority of tektite shapes, though not all of them. It is pertinent to note further that for various reasons Sun's computations overestimate the severity of the aerodynamic forces tending to disrupt the molten material passing through these gases. First, his computation of the amount of material vaporized when Tycho was formed is based on experiments from nuclear explosions which were contained entirely underground. Hypervelocity impact experiments would provide a more realistic basis for such computations. On the basis of a recent paper by Gault and Heitowit [1963], my estimate of the fraction of material which would be vaporized in a hypervelocity impact is very much smaller than the values assumed by Sun. Gault and Heitowit point out the relatively common misconception that large quantities of material are vaporized during hypervelocity impact. Second, Sun has not considered that both the gases and the fused material move outward from an impact and that this common relative direction of motion thereby reduces the relative aerodynamic velocity and therefore the aerodynamic pressures tending to disrupt fused material. Third, the fused material at the instant it jets from the crater need not be composed of the myriads of small individual forms that eventually solidify

as primary tektite forms; it may be much larger masses of fused material which are not fully disrupted by their passage through the crater gases, but become disrupted after passing through these gases by means of their own internal motions combined with other physical processes, such as boiling or degasing. (As the impact gases expand into a vacuum, they cool rapidly, slow down quickly, and would be soon overtaken by the masses of fused material which move at nearly constant velocity.) Fourth, Sun's computation that a mass of molten glass of about 1 to 2 centimeters' diameter would solidify from an initial temperature of about 2000°C in a period as short as 2 seconds, appears, according to my calculations, to considerably underestimate the time required for solidification. It seems to me, therefore, that a really reliable analysis of the complicated physical phenomena involving the formation and passage of large quantities of fused material through the gases produced by the impact would have to consider many things not considered by Sun and would have to be based on more pertinent experimental data than those from underground nuclear explosions.

c. In regard to the comment that Adams and Huffaker have arrived at different entry velocities and entry angles, and that therefore the velocities and angles which we arrived at are questionable, it should be observed that there are several clear reasons for these differences. Our analysis considered three independent types of observational aerodynamic evidence, employed directly measured physical properties in the calculations, and was checked by extensive experimental investigations. Adams and Huffaker, on the other hand, considered only one type of aerodynamic evidence, employed estimated physical properties, and did not conduct any experiments to check their procedures. The only observational aerodynamic evidence they considered was the amount of ablation, but it is not possible to determine the entry velocity from the amount of ablation alone, since this particular evidence only shows that the entry

velocity was somewhere in the broad range between about 7 km/sec and some value greater than 20 km/sec; and as far as this particular evidence goes, there is no major difference between the two aerodynamic analyses. However, by assuming that the tektites formed as ablation drops from a parent body in a grazing entry—that is, by assuming that the entry trajectory was nearly horizontal—they were forced to drop the entire velocity range above the earth escape velocity of 11.2 km/sec. We have found no observational or experimental aerodynamic evidence which supports such an assumption; in fact, our ablation experiments show that this assumption is incompatible both with the evidence from the systematically distorted striae near the tektite front face and with the evidence from the particular spacing between the ring waves on this face. It is these latter two independent types of experimental aerodynamic evidence which we employed to determine the entry velocity and entry angle, and which Adams and Huffaker did not consider. Moreover, they overlooked in their analysis the effect of pre-entry turning on the amount of ablation. Such an oversight leads to entry velocities that are too low. The observational evidence for pre-entry turning of the australites has been recently documented in the paper to which Sun referred [Chapman, 1963]. In view of these evident differences, I see no basis for questioning the entry velocities and the entry angles we have determined.

REFERENCES

- Chapman, Dean R., On the unity and origin of the australasian tektites, paper presented at 2nd International Tektite Symposium, Pittsburgh, Pennsylvania, September 5-7, 1963.
- Gault, Donald E., and Ezra D. Heitowit, The partition of energy for hypervelocity impact craters formed in rock, *Proc. Hypervelocity Impact Symp.*, 6th, Cleveland, Ohio, April 30, May 1-2, 1963, vol. 2, part 2, 1963.

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